animals living in it, or by coral sand carried into it by wind and waves from the outer edge in the same space of time, and therefore I think the balance of evidence is in favour of Mr. Murray's explanation of lagoon formation.

Could the experiment be made, it would be a matter of extreme interest to know if, and in what proportion, carbonate of lime really exists in lagoon waters, as also the proportion in the waters outside the reef, where new coral formation is actively at work.

It is quite reasonable to suppose that the dead coral so dissolved in the formation of lagoons is carried out as material for fresh coral growths. ROBERT IRVINE.

Royston, Edinburgh, March 6.

IN reference to the interesting discussion on coral formations which has recently appeared in NATURE, a few words from the chemist's point of view may not be out of place.

For some time past I have been endeavouring to satisfy myself regarding the solubility of calcium carbonate in sea-water, and with this end in view I immersed weighed pieces of dead coral (dried at 212° F. till constant) in sea-water. These were protected by suspending them under glass bells floated in about 18 inches of water, and allowed to remain at rest for a known length of time. The following are the results obtained:—

First Experiment.—Oculina varicosa, from St. Thomas, West Indies, weight 16·3164 grammes, with a surface of, roughly, 8 square inches, lost by solution in twenty days, 0·0748 gramme.

Second Experiment.—Madrepora scabrosa, from Levuka, Fiji, weight 21·8540 grammes, surface of 16 square inches, lost 0·1497 gramme in thirty days.

Third Experiment.—Montipora foliosa, Amboyna, weight 15·3334 grammes, surface of 15 square inches, lost 0·1223 gramme in forty-six days.

Every care was taken that the corals should not be subjected to the action of other than convection currents. The temperature ranged between 30° and 40° F. Specific gravity of the water found less than 1·026.

Mr. W. G. Reid, in a paper communicated to the Royal Society of Edinburgh, showed that the solubility of carbonate of calcium increased with pressure; and when determining the percentage of $CaCO_3$ in certain soundings I found that the greater the depth the less carbonate derived from surface shells was present, while it is a well-known fact that in the red clay or other deep-sea deposits, $CaCO_3$ almost completely, if not entirely disappears, as has frequently been pointed out by Mr. Murray.

From the above considerations there can be little doubt that there is considerable action going on in the waters of the ocean.

Take, for instance, a circular lagoon four miles in diameter; this would give a superficial area of 12½ square miles. Taking the results obtained in Experiment 1, and applying them to this imaginary case, then in twenty days, in absolutely still water, there would be dissolved 464 tons of $CaCO_3$, equal to 8472 tons in a year. If the specific gravity of carbonate of calcium be taken at 2·65, this amount would give a thickness of half an inch covering the whole area of the lagoon. In other words, at the same rate it would require about a century to deepen the lagoon one fathom.

These results must be, however, very much under-stated, as the temperature in the coral regions is about twice what I could obtain; the sea-water is denser; there is the action of carbonic acid gas, CO_2, which is constantly being generated by decomposing organic matters, especially in these warm areas, and all which would increase materially the solubility. Moreover, there are the tides and currents continually replacing, or rather mixing with, the denser waters.

The coral animals in the lagoon are, however, constantly laying down new material in the shape of $CaCO_3$, assimilated either directly from the sea or through the medium of other organisms upon which they feed, or both combined. Now it depends upon the excess of the one process over the other whether there be an increase or decrease in the depth of the lagoon.

Growth is restricted mainly in reefs to the outer periphery, leaving large spaces of coral sand in the interior to be freely acted upon. In this way the coral formation increases outwards, while there is a deepening of the interior, albeit this deepening is very small. JAMES G. ROSS.

14 Argyle Place, Edinburgh, March 10.

CAPTAIN WHARTON in his interesting paper satisfactorily explains a condition of reef-growth previously little known and but imperfectly understood. I was pleased to learn that Mr. Bourne's long-expected account of Diego Garcia will soon be published. His remarks concerning the *directing* influence of currents agree closely with those of Semper ("Animal Life," vol. xxxi. Internat. Sci. Ser. p. 228). Of the importance of this agency there can, I think, be no doubt; but solution is also an important agency within the lagoon, and one more capable of actual demonstration than the directing force of the currents. But amongst the supporters of the *anti-subsidence* theory of Murray there is a difference of opinion as to the relative importance to be attached to solution; and we cannot accept the name of "theory of solution" for the new view if it is intended to exclude the other agencies that previously cause the death of the coral, such as the repressive influence of sand, the diminished food-supply, the tidal scour, &c. With this exclusive meaning, the name "theory of solution" would contradict itself, and we should be regarding the problem in much the same light as if we were solely to contemplate the mystery of our own existence from the point of view of the undertaker.

The development of the new theory should be borne in mind. Chamisso, seventy years ago, advanced the view that an atoll owes its form to the growth of the corals at the margin and to the repressive influence of the reef-debris in the interior; but this view gave no satisfactory explanation of the foundation of such a coral reef, and Darwin was driven to his theory of subsidence. The great defect in the view of Chamisso was, however, removed by Murray, who supplied the *foundation* of an atoll without employing subsidence; and investigations in the Florida Sea and in the Western Pacific have confirmed his conclusions. The *forms* of reefs he attributed to well-known physical causes; but Semper and Agassiz have dwelt upon the importance of other agencies, and in our present state of knowledge it will be wisest to combine in one view the several agencies enumerated by these three naturalists as producing the different forms of coral reefs. On the outer side of a reef we have the directing influence of the currents, the increased food-supply, the action of the breakers, &c. In the interior of a reef we have the repressive influence of sand and sediment, the boring of the numerous organisms that find a home on each coral block, the solvent agency of the carbonic acid in the sea-water, and the tidal scour. These are all real agencies, and we only differ as to the relative importance we attach to each. Future investigations will probably add others to the list, besides ascertaining the mode and degree of action of each cause.

March 10. H. B. GUPPY.

Reason and Language.

THE kindness of Prof. Max Müller's reply I recognize with pleasure but without surprise, since those who know him know him to be as remarkable for his courtesy as his great learning.

In answer to his first question, I must say that I made a point of attending his Royal Institution lecture on the day his "Science and Thought" was published, and was greatly disappointed that illness hindered my attending the others. But I immediately obtained his book, and applied myself to understand what seemed to me its essence, though I have not read it from cover to cover. Should I have to review it, of course I shall conscientiously peruse the whole of it.

Before replying further, it may be well to restate my position as follows.

Man is an intellectual being able to apprehend certain things directly and others indirectly. Normally, his conceptions clothe themselves in vocal sounds, and get so intimately connected therewith, that the "word" becomes practically a single thing composed of a mental and an oral element. But these elements are not *identical*, and the *verbum mentale* is anterior and superior to the *verbum oris* which it should govern and direct. Abnormally, conceptions do not clothe themselves in oral expressions at all, but only in manual or other bodily signs, and this shows that concepts may be expressed (however imperfectly), in the language of gesture without speech. One consequence of these relations is that neither the utterance of sounds (articulate or inarticulate) nor bodily movements could have generated the intellect and reason of man, and Noiré's hypothesis falls to the ground. On the other hand, beings essentially intellectual, but as yet without language, would immediately clothe their

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nascent concepts in some forms of bodily expression by means of which they would quickly understand one another.

As to the expressions "reason" and "reckoning," I would observe that a study of an organism's embryonic development is a most valuable clue to its nature, and no doubt a similar utility attends historical investigations in Prof. Max Müller's science. Nevertheless, we cannot understand the nature of an animal or plant by a mere knowledge of an early stage of its existence; an acquaintance with the outcome of its development is even more important. Similarly, I venture to presume, the ultimate meaning of a word is at least as much its true meaning as is some archaic signification which may have grown obsolete. The word "spirit," if it once meant only the breath, means more now—as we see from the Professor's first letter. Similarly, if "reason," in its Latin form, once only meant "reckoning," that is no "reason" why it should only mean reckoning now. Here it would seem as if we had an instance of the *verbum mentale* having acted upon and modified the *verbum oris*. I cannot but regard the representation that affirmative and negative propositions are mere cases of addition and subtraction as an incorrect and misleading representation, save when they refer to mathematical conceptions. I am compelled also to object to another of the Professor's assertions. He says:—"There is a wide difference between our apprehending our own activity and apprehending that A is A. Apprehending our own activity is inevitable, apprehending that A is A is voluntary." It is true there is a great difference between these apprehensions, though they both agree in being instances of apprehensions which are not inferences, and as such I adduced them (NATURE, February 16, p. 364). Nevertheless in my judgment the difference between them is not the difference which the Professor states. Both are alike voluntary, regarded as deliberate reflex cognitions, and both are alike inevitable, regarded as indeliberate, direct perceptions. The labourer inevitably perceives that his spade is what it is, though the nature of that perception remains unnoticed, just as he inevitably perceives his own continuous being when he in no way adverts to that fact.

I must further protest against the assertion that the idea "therefore" is "present in the simplest acts of cognition"—that every perception of an object is an inference. This I regard as one of the fundamental errors which underlie all the madness of idealism. Akin thereto is the notion that a philosopher who desires to speak with the very strictest accuracy ought, instead of using "the big I," to say, "a succession of states of consciousness." To me it is certain that even one state of consciousness (to say nothing of "a series") is no more immediately intued by us than is the substantial ego; each being cognized only by a reflex act. What I intue is my "self action," in which intuition both the "ego" and the "states" are implicitly contained, and so can be explicitly recognized by reflection. I was myself long in bondage to these two errors, from which it cost me severe mental labour to escape by working my way through philosophical subjectivism. These questions I cannot here go any further into, and I only mention them in consequence of Prof. Max Müller's remarks. I will, however, in turn, refer him to my "Nature and Thought," as well as to a larger work which I trust may before long be published, and which, I venture to hope, he will do me the honour to look at.

My object in calling attention to the fact that one word may have several meanings, and several words one meaning, was to show that there could not be "identity" between thought and language. This point the Professor seems practically to concede, since he now only calls them "inseparable, and in one sense identical." I do not understand degrees of identity. No mere closeness of resemblance or connection can make two things absolutely identical. I did not, however, content myself with denying this "identity" on account of polyongeny and homonymy; I also referred to common experience (which shows us that men do not invent concepts for preformed words, but the reverse), and I appealed to certain facts of consciousness. To my assertions about consciousness the Professor replies: "The object of all scientific inquiry is the general and not the individual." But this is a quite inadequate reply, since our knowledge of general laws is based on our knowledge of individual facts, and if only one man could fly, that single fact would be enough to refute the assertion that flight is impossible to man.

With respect to evolution, I never said that Prof. Max Müller misunderstood "natural selection," but only that he misrepresented it—of course unintentionally. It is of the essence of natural selection not to affirm teleology as formerly understood, although, of course, it can say nothing (for the whole of physical science can say nothing) about a primordial teleology at the foundation of the entire cosmos. I, in common with the Professor, look forward to "the ultimate triumph of reason and right," but my confidence is not due to any "faith" I have in "Nature" or anything else. I profoundly distrust "faith" as an ultimate basis for any judgment; I regard my conviction as a dictum of pure reason—the certain and evident teaching of that science which underlies and gives validity to every other. I therefore agree with Prof. Max Müller in regarding it as a lesson which "true philosophy teaches us."

ST. GEORGE MIVART.

Oil on Troubled Waters.

IT may interest some of the readers of Captain Wharton's paper on this subject to have their attention called to a curious narrative in Bede, illustrative of the power of oil over troubled waters. When a certain presbyter, Utta, was sent from the North of England by Oswiu to fetch his bride from Kent, he applied to Aidan, the greatest teacher of his day, for his blessing. Aidan gave him not merely his blessing, but some consecrated oil, and told him that on his way back from Kent by sea he would encounter a storm, and thereupon he was to pour the oil on the sea, which would immediately become calm. It happened as St. Aidan had foretold. Utta and his fair charge were duly overtaken by a fearful tempest; the waves were breaking over the ship, when Utta bethought himself of Aidan and his oil. "Assumpta ampulla, misit de oleo in pontum, et statim, ut prædictum erat, suo quievit a fervore" ("Historia Ecclesiastica," lib. iii. cap 15). Aidan had been brought up at the monastery of Iona. Did the boatmen of the Western Islands in the seventh century know of this use of oil? and did Aidan bring the knowledge from thence that saved from shipwreck Utta and the bride Eanfleda? EDW. FRY.

Were the Elephant and Mastodon contemporary in Europe?

ONE of the most effective services which NATURE does for the cause of science is to enable students who live far apart to exchange ideas in its correspondence columns. May I be allowed to ask a question of some interest, perhaps, to others besides myself? It is a singular fact that we probably know less of the *sub-aërial* conditions prevailing in so-called Pliocene times than we do of those of most geological horizons. The marine Mollusca of this age have been preserved in large numbers and in many places, but the remains of the land fauna are singularly sporadic and broken.

I know of no fragment of a land surface of this age which exists in Britain. In the Crags we have a very puzzling medley of mammalian bones and marine shells mixed heterogeneously, and pointing unmistakably to the beds having been rearranged, and, as the French say, *remanié*.

Unfortunately the Pliocene period has been largely defined on the evidence of these very unsatisfactory beds—unsatisfactory not merely because *it is certain* that the remains of land and marine animals are confusedly mixed up in them, but also because *it is exceedingly probable* that the debris of two geological stages have been mixed together also.

It seems clear to me that, if the Pliocene age is to be clearly defined, we must not rely upon the evidence of the English Crags for defining it, but go elsewhere—namely, to France, Italy, &c.

It is very well known that nowhere in France has the mastodon, which is generally accepted as a very typical Pliocene mammal, been found in the same beds with the elephant. In the English Crags, no doubt the older type of elephant (the *E. meridionalis*) and possibly also molars of the later forms (*E. antiquus* and *E. primigenius*) have occurred with mastodon remains and the remains of other so-called Pliocene beasts; but the mixed character of these deposits puts them out of court, and we are bound to follow the evidence of the French beds, which occur *in situ* and unmixed, if we are to be assured of our position.

My purpose in writing is to ask whether the Italian evidence is the same as the French. Unfortunately the Italian beds do not seem to me to have been studied with the minute care which they deserve. No doubt enormous numbers of mastodon remains and also of remains of *E. meridionalis* occur close together in Italian deposits, but so far as I know the question has not been critically tested as to whether they occur in the *same* beds

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